

Zuckerman

DATE: February 14, 1977

TO : File /HAZ

FROM : Joshua Lederberg

SUBJECT: Further critical comment on <sup>"</sup>Scientific elite.<sup>"</sup>

Page 92: "Scientists converge": this gives too little credit to the very complex process of department building: besides the undeniable role of mutual gravitation which is reflected both in some initiative on the part of department member scientists and their ability to provide incentives for others to move, there is a much larger complex of issues including personal leadership but also the historical context of the department and the university. It would be interesting to know how many of the prize winners had been department heads at various stages in their careers. And in such a catalogue I would be careful to discriminate the different functional roles of a department: there is a huge difference between a very small department ~~and~~ (as Genetics has been, in a way an extended laboratory,) and some of the very large departments of chemistry which are a real chore to administer.

Do you happen to know any of the detail about the Zoology Department at Indiana? I would be surprised if Muller played very much of an active role in department building although he was, of course, a great attraction.

It would also be a good place to recall the remarks you make elsewhere about the different visibility of various fields to the Nobel Committees. Altogether I very much enjoyed this chapter, particularly the discussion of apprentices and masters. Perhaps I would have asked you if you knew more about the way in which the apprentice was able to reach the master: the facilitating role of earlier teachers and inspirational figures - like Ryan in my own case and Strandskow for Watson.

Page 101, "few young scientists who did not develop into reasonably good scientists". Well, that has to be true in a relative ~~sense~~ for these figures to be able to work in the system. But is that just a speculation on your part: do you have any data on the frequency of washouts? (Or I have I had more than my own share, possibly for not having been ruthless enough in initial selection and subsequent sustenance?)

Page 101, confidence in work. Work plays such a large role in the overall personality of the young scientist that learning how to integrate this in one's life and developing some overall sense of self-confidence and how to live as a scientist in a world that offers intermittent incentives and discouragements is as important a role for the master as the specifically work-oriented factors. To what extent do these masters come out as "father images" in this more general sense in your inquiry?

over

Page 117. Of course you know that Muller was notorious about his jealousy about priority. To what extent this fed into his comments about Morgan; to what extent his experience with Morgan had bruised him, I leave you to judge. There of course are many other elements of Muller's career that speak to his idiosyncrasy.

Page 126. About recruitment to top-rank universities and class origin. Some element of this may have to do with snobbery on the part of the scientist. There was a time when Indiana offered more substantive advantages for experimental biology than did Harvard. If I had been a Harvard graduate, rather than having come from the lower <sup>class</sup> university that I did, I might have found it very difficult to resist their blandishments in favor of staying at Wisconsin.

For some part of the relevant period the still manifest anti-semitism of some of the schools has to be factored into the discussion of effects of class-origins.

Page 135. Here ~~you~~ talk about the prize-winning work as if it can be identified with precision. This contradicts some of your interview material where you question the extent to which the asserted work for which the prize was given was proxy for a more complex background.

Generally, with you emphasis on the achievements of Jews it might be worth to reflect back that the Nobel Committee itself is certainly not preponderately Jewish in order to satisfy some phantasies of conspiracy that I am sure still linger. It is of course true that in Sweden also the rather small Jewish community is also very heavily represented in its academic elite. Even a fairly small absolute representation could be fairly effective in hindering covert anti-semitism in the choices of nominees.

Page 100. I do not know how atypical my socialization experience was. As you know, Ryan was my principal source although I have to say that much of the technical content that he was able to convey to me he had received in turn from Tatum. What do other respondents say about the further role of less visible figures, who are not necessarily always great investigators?

Page 164: about M.D.'s in the Academy. You are probably right about the attribution about "less rigorous and fundamental", but this is mediated by the bureaucratic structure of the Academy, the question of which one of the sections has responsibility for the fields of medical work. Generally speaking, the Academy just did not think it was its business to focus much attention on medicine or agriculture or even engineering, even though there was some token representation of those areas in the Academy's structure.

Oage 164 (footnote 9): delayed recognition by the Academy. This would be a good focus for your interests in discensus. Why not inquire more concretely about the extent of differences of opinion about the significance of the work in question. One of course also has to ask about the role of personal controversies and the accidents of personal membership in matters like this too.

Page 166: (and later on also 199). I would be interested in the extent to which successful and self-confident scientists were very reticent about listing honorifics long before they won their prizes.

2/14/77

I can tell you a little more about the dynamics here. I think it was my secretary but it may have been someone else who suggested that I was displaying some reverse snobbery to a conspicuous degree by my own pattern of listing although this was consistent with my life-long practice. With respect to secondary honors there was the problem of discrimination: if these start accumulating you (anticipate) running into trouble if you list some and then not others. As I began to get a couple of honorary degrees, and knowing how some of my well-wishing friends had worked hard to arrange those, it became increasingly difficult to follow my own instincts in that matter. (As you know, I am really not a modest person but I greatly prefer the opportunity for substantive self-expression to the cataloguing of formalities). Most recently I have faced the allegation of concealing my non-academic interests and have therefore felt impelled to put my corporate connections also on the record. So, I am afraid you will see the next editions of the directories still further encumbered. I suspect your indexing of awards is a considerable underestimate and in a biased way as far as the laureates are concerned.

*but older (mostly prewar)*  
I must say, that when I visited Harold Uri's<sup>ey</sup> office not too long ago, I was deeply impressed by the showering of awards that were displayed at the University at La Jolla there. I had to reflect that a great many of them were international and reflected an ~~era~~ of recognition of U.S. preeminence that may be somewhat less likely to be exhibited today. But this is not to take anything away from Uri's astonishing role in science over the decades.

Somewhere else you talk about the cash value of the spill-over effects of the prizes. I hope Uri is right and that it turns out to be hard cash. I know one of the enterprises that has exploited him in the most mercenary way is much more pie-in-the-sky than he himself realises. I would think (and hope!) that it is true that a laureate can have some confidence in not being allowed to go quite hungry and that the prestige connected with the prize will always offer some opportunity, for example to continue working after retirement and matters like that. I wonder if you have any statistics on that point. I would be quite surprised if it turned out that there was a significant increase in life-time earnings on the part of laureates compared to their near peers; depending on the precise field I would expect that the prestige of the prize acts as a compensation for and mild disincentive for the extra effort involved in higher outside earnings. These data are not easily obtained, but you might make your own guesses, say from the circle of your own colleagues, about the correlation of earnings with academic esteem. The prize is a non-standard element but I do not think it will disrupt the entire trend.

*vs concentration*  
Page 167, top, accumulation of advantage. I do not see how the statistics prove an "accumulation" of advantage without more explicit evidence that one advantage directly leads to another rather than both having been derived from a common course. More on this later.

Page 167: offices etc. "elect to transform the esteem in which they are held into positions". That is rather peculiar language. For most of us the problem is to justify limiting commitments in a direction that we know we have some responsibility for. At a certain point when being busy is legitimately expected it gets a little easier to turn these things down: your correspondents probably even exaggerate how difficult it would be to fit things into a schedule they then know is crowded.

over

2/14/77

Chapter 7, ambivalence and surprise<sup>1</sup>. I think most laureates would be very grateful to you for collecting this information and helping to show how widely our peculiar concerns have been shared.

Reading this chapter made me reflect further on why I was so surprised in 1958. It was not modesty about my own achievements or where I stood in relation to the scientists I knew. But I had not thought very much about it and believe that prizes were things that came after another 30 or 40 years of longevity and that there probably were a number of candidates who would have to be thought about first simply because they were older. There had not been very many prizes in genetics in the recent past, and I took it for granted that people like Beadle and Tatum and Luria and Delbrück and Burnett would have to have their turn first before there would even be a question about the generation in which I stood. I knew very little about the mechanics and social network that was involved in the formulation of the prize and thought that was a much more nearly accidental and formal recognition than it turns out to be the case. If I try to reflect a little more deeply in a psychodynamic sense, I would have to say that age differentials have been, or at least at that time were, rather significant orienting factors for me.

I cannot recall ever having heard anyone else suggest that I ought to be alert to the possibility of getting a prize in the near future, or ever for that matter, although this would be hard to guarantee. Anyhow, I was mercifully spared having to even think about it before the event.

Page 205: prizes and problem choice in science. I do not think there is a great deal of first-order influence, although the existence of prizes must offer some accentuation to looking for the bold strokes of singular discovery, which on the whole I would feel to be quite functional. The logic of the previous paragraph could be inverted. In fact, as far as practical problems go, in medicine examples like ~~Waksman~~ point in the opposite direction.

Undoubtedly, there are effects mediated in a more complex way: public visibility and the support of fields must surely be influenced by the field-distribution of the prizes. The prize-giving system itself affords an apparatus of critical judgment which is sadly lacking in some fields, for example agriculture. The prize system has, I think, also justified Nobel's intention of highlighting the relationship between basic research and visible and thereby important discovery.

Page 207: I suppose if the prizes did not speak to some fairly deep-rooted sentiments about the nature of the scientific process, they could never have reached the preeminence that they have. This may bring us back to the question of what kind of elite the Nobel laureates are: I suppose some phrase like "grudgingly respected and legitimate notoriety" might capture some of the ambivalence about it. The greatest dangers about the prize would have ensued if there were substantial distortions to stress the integrity of the prize-winning process. To the extent that the committee has been able to resist concerted lobbying and politicking and extraneous pressures, and to the extent that scientists channel their competitiveness to the central norms of science, it is hard to make a case that the prize system is all that bad. I imagine if it did not exist something else would have evolved to take its place and who can be sure that we would be as fortunate about its integrity.

over

2/14/77

*at Stockholm*  
I made my own speech about the function of the prizes. And what I said there about the international role, I think still hold. One would have to look a little more deeply to see if, as I would speculate, science in the Soviet Union has been constructively impelled to be more internationally directed is in fact the case.

*the*  
Whether a prize system is good for Sweden and for Swedish scientists is another question. It has certainly enhanced their smugness to be in a position of being the arbiter of these global awards. And while the prizes to Scandinavians have generally been reasonably respectable, there is still no question of some statistical bias in favor of their compatriots. This is relatively harmless except insofar as it may inaccurately inflate the self-appreciation that Swedes have about their role in world science.

On a larger political plane it is just as well that it is the Norwegians, not the Swedes who control the peace prize. There are lots of problems about Swedish neutralism, as it is; although one can sympathize with the good fortune of a people that has been able to get away with that posture as long as they have.

Page 209 - top paragraph. Accumulation of advantage again. "cast considerable doubt that differences in performance reflect equally marked differences in initial capacities" is a carefully drawn statement that is hard to quarrel with. It lets different readers get what they want to see out of it. I wonder if you would be willing to quantitate that remark, however. Just to try to pin you down on exactly what you mean by it, I would ask you to give me some sort of graphic representation (or do this any other way you siwh) of what you think is the functional relationship between "initial capacity to do scientific work" as an independent variable and difference in performance as the dependent one. If you also like to suggest what you think the regression coefficient is between these two measures, the extent to which accidental factors diminish the functionality or would disturb the rank order relationship between the input and output - so much the better. My own general expression is that while this phenomenon deserves further attention, tending to be neglected in other discussions, that you are making a number of speculative allegations that deserve to be stated in somewhat more concrete form and perhaps need further validation.

We could proceed to some further models on the social utility of different forms of these relationships, and I suppose one would have to make some further assumptions about the relative efficiency with which investment is transformed into socially useful outputs.

Part of my irritation may come from connotations of "accumulation of advantage" that are not intended in your use of the phrase ~~and the~~ strictly descriptive way.  
*by a*

And of course I will understand why it might be predicted that I would react in this fashion.

Page 211: "social and psychological processes thus limit the accumulation of advantage". Again from some personal introspection, the achievement of a prize is a consumation like climbing Everest. As far as external social reinforcements, there is no further to go. Being believed of that external structure can be quite threatening. At minimum it means a return to some deeper seated self-motivation and I do not see how this can help but lead to a certain degree about introspection concerning fundamental personal aims and motives. Then quite apart from all of the external demands for commentary and participation in more general issues, there is an internal process that then tends to reduce preoccupation

over

2/14/77

with the very highly specialized issues that is so indispensable for what we usually regard as scientific advance. Some of this discussion should also be related to the questions about father images that I raised earlier.

Returning briefly to accumulation of advantage, I have put together a couple of crude sketches to illustrate what I mean about trying to lay on some graphic model of the extent of discrepancy that you are hinting at at the top of page 209. These sketches do not capture the covariance terms of the relationship of advantage to merit nor the problematics of definition of those variables, but I think they do reflect a worthwhile step in concretizing your ideas of what you were trying to say. The whole issue deserves a more analytical economic treatment. For a beginning on a somewhat related theme let me suggest to you a short booklet by Arthur M. Okun, "Equality and Efficiency: The Big Trade-Off", Brookings, 1975. But I hardly have to tell you where to get economic expertise.

Appendix A. This chapter was really fun! I was interested that you think there is much carry-over between your interviewing structure for this particular elite; I think there would be pros and cons to your exchanging places with Candice Bergen.

That aside, your strong point was of course that you are dealing with an elite which is also a group of your colleagues in science. I have no other way to account for what seems to be an astonishingly high rate of compliance. Even the more so, if you were still working on your dissertation when you were setting up those interviews! I would have been less surprised had you been able to come on at your beginning of that effort with some history of publication and some well defined theoretical structure in which you were pursuing your tasks. If I can find my own correspondence with you, I will see if I can reconstruct the circumstances of my own response to your solicitation for an interview.

I guess in retrospect it is too bad that you did not have the *context* of discovery as much on your mind as the issues of cooperation and conflict. But of course I am sure there will be still other questions to be thought about in the future.

*Kee you* Momentarily back to page 212: I am not sure exactly how guarded you meant that last paragraph to be. ~~I am~~ implying that the performance of the 41st chairs is significantly greater than the group who were named? Again, there is a quantitative issue, lurking here behind your qualitative reservations, and the extent of them is going to be interpreted very differently by different people given the language that you use. Are there other systems of recognition and reward that you can point to as operating either more or less efficiently than this one?

Back to the Appendix A, page 224: I am a little surprised you did not use at least implicit letters or remarks of reference from "satisfied clients" as ways to enlarge your entree to new subjects. I am not surprised at their satisfaction and I would have thought the greatest difficulty would have been in getting started. But I guess you just did not find that necessary.

Page 228. Perhaps a lot has happened in 14 years: your own rise in status, self-confidence, and competence, as well as what I expect is a bit more candor and less stuffiness in the system. But I have the impression both from what you write here and from, say, the Tatum interview that you could have pressed harder than

2/14/77

you did, once you had established your credentials as a scientist and that you were not on an idle mission merely collecting gossip. For example, (and these are things to do only at the end of a discussion when there was not much to lose,) you might have remarked to some of your more reticent interviewees "Don't you think it is important to get the facts on the record? I can understand that you might wish to be careful about what you say to someone like myself at the present time, but have you taken your own precautions to record your unique observation in a way that will help future historians?"

The value of such communications, besides the obvious instrumental one of helping to be sure that there will be such archives, would, I think, ~~to~~ be to encourage such individuals to confide in you even further than they had before. I can see that you used many of your resources of personal honesty and freshness and the solid evidence of your commitment by the homework you did (which is astonishing!). I do not have the sense that you really deeply engaged your respondents in the analytical process in which you were taking part.

Harriet, this is no criticism of the way that you conducted your affairs at that time! But I think it would be constructive to put some remarks like this on the record to give your colleagues of the future, reading these, some more courage to take somewhat stronger initiatives.

About the 41st chairs, as I am sure you are well aware one of the most interesting questions is "what about Salk and Sabin?" In fact, haven't you noticed how often Salk has been attributed with a prize that he was never awarded! Some of this may be requotation from original random errors in newspaper reporting. (And you might also want to look into something called the Noble Prize.) It really must still make Jonas wince not a little when he reads things like that. You may also know that he has been reinitiating a campaign for reconsideration of killed versus the Sabin vaccine. I am sure that he feels that justice and health would be more likely to be on his side if he had also had the right kudos.

2/14/77

Postscript to notes on the Scientific Elite

It is too bad that a serious-minded study of this kind needs to be burdened with this additional thought, but I predict that you too are going to wince when book reviewers extract your remarks about accumulation of advantage, quote and extrapolate them out of context, and use them to build a case that the elitist system of science is nothing but that self-aggrandizement.

qu I do not find any 1963 correspondence in my file. (Nor do I recall it). However, that was a particularly troubled time for me, personally, and I might have been even brusker than usual and sent you nothing but a perfunctory reply that put even you off from making further inquiries. For various reasons that was a time I certainly did not want to go into issues connected with public visibility.